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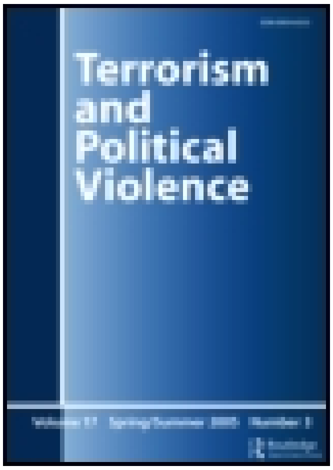

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Does a coercive official response deter terrorism? The case of the PLO

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Does a Coercive Official Response Deter Terrorism? The Case of the PLO

VICTOR T. LE VINE

BARBARA A. SALERT*

We here revisit the recurrent question whether vigorous official anti-terrorism measures – 'coercive response' – have a measurable deterrent effect on the incidence of terrorism. By using several regression models to overcome serious problems of event-measurement and time-series analysis, we analyzed over 600 incidents of 'overseas' (outside of Israel) PLO-directed or PLO-sponsored terrorism and the official responses to them during the 1968–86 period. We concluded that such deterrent effect(s), where they were statistically significant, tended to be visible only for relatively short periods after the terrorist events themselves, but were nonetheless of such minimal impact as to suggest that official coercive responses had more use as political defenses than deterrence.

This article examines the impact of coercive official response on the overseas terrorist activities of the Palestinian Liberation Organization (PLO). These activities have been concentrated in the West, where ordinary people became the target of violence designed to secure ends with which the victim had little connection. Some have argued that citizens of Western democracies are particularly likely to be targets of terrorist attacks both because they live in countries that cannot mount the kind of repressive effort that would be necessary to deter terrorists, and because the West makes itself more vulnerable by the enormous attention terrorism receives in the press, from government officials, and as a salient issue of public concern.[1] Public attention may yield intangible payoffs to terrorists in the form of recognition, or more tangible benefits, such as the release of previously

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* Barbara Salert died in March 1993. At the time of her death we had been working on this study, and had anticipated completing it late that year. The work lay fallow two years for entirely personal reasons. However, given the recurrent interest in the issues we address and because I think that our approach offers some alternative solutions to and explanations of the problem, I decided to complete and update the MS. Considering these circumstances, I forewent the convention that dictates the invariable use of the present tense in presenting joint views and analyses. Where additions clearly reflect work and analyses done after March 1993, I have so indicated, or added a signed note. John Sprague reviewed the methodological aspects and I am grateful for his helpful suggestions and criticisms. VLV

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incarcerated terrorists, while the lack of repression makes it easier for terrorists to operate freely. Untutored intuition would suggest that both official coercive response and attention affect the level of terrorist activities. The former, however, is more amenable to government control than the latter. Unfortunately, other than the finding of Enders and Sandler that major antiterrorism measures tend to deflect terrorist activities to other venues,[2] there is little evidence on the overall efficacy of such control efforts.

Apart from its intrinsic interest as the major, and most effective Palestinian organization, the PLO has several characteristics that make it a suitable subject for a case study of this problem. First, it has always been clearly a political group with objectives having the support of a wide constituency. As such, there has been a wide arsenal of strategies at the disposal of PLO leaders. If terrorism were chosen (at least until December 1988, when Yasser Arafat unequivocally rejected terrorism as PLO strategy), there was presumably always a reason for the choice. Second, the group (or its factions, elements, affiliates, and allies) has used terrorism to secure political objectives. This means that, in addition to the eminently practical concern that these activities are there to be studied, we can use the PLO to assess the impact of official coercive response on terrorism that is both (a) political and (b) strategic. While the conclusions may not apply to groups that do not have much if any of a constituency, it is likely that assessing the impact of coercion on PLO terrorism will shed light on the effectiveness of such means of confronting at least this form of terrorism.

The reason we choose to concentrate on the activities of a reasonably well-defined group, as contrasted to a statistical study that combines the activities of all groups in a single data set, as do Enders and Sandler,[3] is that, like them, we start from the premise that terrorism is a strategy, but argue that that premise leads to a different analytic approach. Whether terrorism is chosen or not should thus depend on an assessment of how well it works in securing political objectives and a consideration of the available alternatives. If we start from the premise that terrorism is a strategic choice, then the relevant unit of analysis is likely to be a particular political group, rather than the terrorist events themselves, and that because conflating choice-theoretic considerations with a broad range of terrorist events misses the point of the specific vectors of choice available to specific actors or groups of actors. We posit that a leader who has to decide whether to send people to engage in terrorism is likely to base his or her decision on the likely consequences to that group, rather than to all groups. A PLO leader with well-trained personnel might not have been overly impressed by the fact that some 'amateur' group was quickly apprehended in some country – he might have reasonably inferred that that was due to their ineptitude. Of course, the leader might have based his calculations on the overall repressive capacities of the target nation. But there would have been no reason for him to weigh the consequences to other terrorists more heavily than, say, of any other form of criminal behavior. Furthermore, once a history of terrorism has been established, the narrowest reference class for him to use would have been the extent to which members of his group could be expected to evade detection, apprehension, incarceration, or death. If this is the case, then the impact of official coercive response on terrorism may not be discernible if all acts of terrorism are grouped together. We recognize, of course, that the PLO consisted of several groups operating openly or covertly under an umbrella organization, and that the reasons one such group used terrorism might not be applicable to another. However, it is neither feasible nor necessary to separate out the component groups for the purpose of analyzing the deterrent impact of coercion. It is not feasible both because the number of cases of terrorism for each PLO member organization would be too small to allow for reasonable analysis, and because some groups set up transient terrorist wings whose links to the parent organization may not have been entirely clear to outsiders – and occasionally, to insiders as well. It is also unnecessary because similarities in training, funding, and logistical provision make it likely that the extent to which authorities were able to apprehend and incarcerate terrorists of one PLO member organization is relevant to the considerations of other PLO groups.

Theoretical Underpinnings

If terrorism is a strategy, the relevance of coercive response is straightforward: any increase in coercion will make the activity more 'costly' and, if other things remain equal, less likely to be chosen. The popular variant of this argument is readily found in the speeches and writings of both conservative and liberal politicians. Its academic counterpart appears not only in the writings of rational choice theorists such as Kirk, Sandler and Tsebelis,[4] but also in the works of social mobilization theorists such as Tilly and Gamson.[5]

Empirical evidence on the impact of coercive response on political violence in general and on terrorism in particular is less clear. Some studies have found evidence of a deterrent effect of official coercion on political violence; others have evidence of 'U' or 'inverted U' relationships between official coercion and violence.[6] The more recent work by Enders and Sandler[7] demonstrates a substitution effect at work; that is, when a relative increase in the costs of one kind of terrorism occurs, terrorists will shift to less costly (though not necessarily to less destructive) modes. This diversity of findings is complicated by the fact that coercive official responses – or anti-terrorist policies – can be treated as a general characteristic of regimes (the extent to which the regime is prepared to engage in repression) or as the actual force directed against a rebel group. Although there are no clear findings for either definition, the multiplicity of concepts here suggests that one reason for the diversity of conclusions is that 'coercion' is a multi-faceted phenomenon. A further complication is provided by the literature on crime, which provides relatively consistent findings on the effects of deterrence: it is the certainty rather than the severity of punishment that deters crime – if it deters it at all.[8] This result may, of course, be applicable only to cases where practical limitations make wide variations in severity of response impossible.

These results suggest that there are at least three factors to consider in assessing the deterrent effect on terrorism of official coercive response: the likelihood that a terrorist will be arrested, injured, or killed; the severity of the sentence if the terrorist is arrested; and the overall 'reputation' of the host country for its treatment of terrorism. Our expectation is that there will be a relatively small, or perhaps even negligible, deterrent effect of official coercive response on the level of terrorism for all of these forms of coercion. The reason for this is that terrorists usually rely on small groups of people to engage in acts that may occur in diverse places and affect a wide variety of people. The small numbers were relevant particularly to groups like the PLO, whose member units had thousands on active duty, and who could recruit replacements from many thousands of additional people. If during a year ten, 20, or 30 people were arrested or even killed, the group has suffered only a small casualty rate relative to other forms of warfare. Since only small numbers of people were ever committed to overseas terrorism, this means that even if governments did manage to arrest or kill all of those who engaged in such acts – a feat that would demand huge resources and may well have been impossible – the leaders of the component organizations might well have decided that the casualty rate still fell within acceptable limits. We should add that it is also the case that where 'suicide' operatives were employed – people willing to blow themselves up with their targets, or almost certainly sacrifice their own lives to the fulfillment of their missions – the calculus of replacement was secondary to the success of the act of terrorism. In general, what usually counted more in these events was the 'propaganda of the deed', that is, the effect of the act itself; less than the availability of replacements or the immediate costs, high or low, of an operation.

This does not mean that terrorists will not respond at all to coercion. The 'costs' of terrorism, in terms of arrests and casualties, are small, but real. If other factors are equal, it is likely that terrorists will choose lower costs if these are available. Thus, if it is possible to engage in terrorist acts in countries with lower arrest rates, or in countries which historically have been lenient in their dealings with terrorists, such countries may be preferred target sites. In other words, even if the aggregate rate of terrorism does not respond to deterrence, it is possible that official coercive response will be effective in securing regional dispersion. It is also possible that increasing the costs of terrorism would have decreased its overall incidence for some time, if PLO organizations had been willing to commit only a small portion of their resources to 'overseas' terrorism. As it turned out, that hypothesis remained untestable because the incidence of PLO terrorism did decrease dramatically after 1988 as a result of a radical strategic shift of the whole organization from terrorism to diplomacy. How much cost factors figured in the decision is unclear, since at the time the organization had ample resources to continue to mount terrorist events both within and without Israel. It likely that a combination of factors, including the eruption of the Intifada in December 1987, consequent changes in Israeli attitudes, and a decline in official Arab support, had a good deal to do with the decision, not to speak of the failure of terrorism to change Israeli policies or bring about the collapse of the Jewish state. In a larger sense, of course, cost considerations undoubtedly went into the decision insofar as the fact of diminishing returns on the investment in a terrorism strategy became part of the calculus.

'Overseas' Terrorism of the PLO

The analysis here is restricted to terrorist activities of the PLO outside Israel and the occupied territories. The reason for this exclusion is, first, that activities in the Middle East, and particularly in Israel, formed part of a guerrilla war that may well have been qualitatively different from terrorism elsewhere and, second, that we sought to ascertain the effects of coercion in a variety of settings. The inclusion of Israel would almost inevitably make conclusions applicable primarily only to the Israeli case, since Israel has been the site of the bulk of terrorist activity. Furthermore, we suspected that terrorism in Israel and the occupied territories was quite different from terrorism overseas. Within Israel and the occupied territories, terrorism (defined as attacks on civilian targets for purposes of intimidation or publicity) may not be viewed as strategically different from guerrilla warfare (defined as attacks on military personnel and economic units). After all, modern warfare has often involved attacks on civilians for what are presumably taken to be good military reasons.[9] In this case, both terrorism and guerrilla warfare within Israel and the occupied territories presumably served the function of legitimation within the Palestinian community and continued pressure on Israel to accede to Palestinian demands. Terrorism outside of Israel was different in that it usually targeted people who (a) were unlikely to suffer enough to pressure Israel to give in to the PLO, (b) were likely to react with hostility to acts directed either against local inhabitants or even against Israelis on soil protected by foreign governments, and (c) might have been expected to exhibit a long-term sympathetic response to the plight of the Palestinians as a result of the publicity conferred by terrorism. 'Overseas' terrorism, in other words, involved both costs (in the form of foreign hostility) and benefits (in the form of international recognition that might have ultimately translated into a certain amount of sympathy) that differed from those of terrorism within Israel, where the benefits were those of a war of attrition that (in the PLO calculus at the time) could ultimately force the Israelis to concede to some form of an independent Palestinian state.

The most complete compilation of overseas PLO terrorism we were able to find is the Israel Defense Forces (IDF) *Spokesman.*[10] We used the Foreign Broadcasting Information Services reports from 1982–85,[11] as well as two sources containing chronologies of transnational terrorist events, to check on the completeness of this source and found some differences but they were not sufficiently large to warrant returning to original press sources from all countries concerned. The IDF compilation includes information about the incident, date, location, damage, and fate of the perpetrators.[12] We also consulted the 1986 study[13] by Merari and Elad on international Palestinian terrorism to cross-check our incident and location counts.

The organizational affiliation of perpetrators is not always known and, when it is, the sources may describe their organizations as affiliates or allies of the PLO rather than as member organizations. For this reason, we created three different listings of PLO terrorist incidents. The first, and broadest, includes virtually all incidents in the IDF listing that were or could reasonably be shown to have been perpetrated by the PLO.[14] The second includes all incidents listed as perpetrated by a PLO organization or one of its affiliates/allies. The third, and narrowest, includes only those incidents that were simply traced to a PLO organization. In each case, we compiled monthly listings of incidents. For the 216 months (18 years, 1968–86) included in the IDF report, we found a total of 556 incidents on the broadest definition of PLO overseas terrorism, 327 incidents attributed to the PLO or an ally, and 264 incidents attributed to a member organization of the PLO. Note that these numbers indicate that throughout the period under consideration, the PLO managed to maintain a significant, active overseas force.

The arrest and casualty figures indicated the problems faced by governments trying to curb this force. The PLO averaged significantly less than one person arrested per incident, with the result that, depending on which category of PLO activity is chosen, the organization lost between 11 and 17 people a year to arrests. Some of these people were not lost for very long as they were merely deported or sent to prison for short periods and then exchanged after subsequent acts of terrorism. If we exclude those known to have served no prison term or prison terms shorter than six months, the loss rate for the PLO drops to about seven to 11 people per year. Even if we add to this the 11 or 12 a year who were wounded or, more rarely, killed in action, the overall casualty figures for the PLO for this type of action are extremely low for a military organization.

Data Analysis

There are two major problems in attempting to assess the impact of official coercive response on overseas terrorism. The first entails finding a suitable model that can be used to obtain statistical estimates of deterrence effects. The second entails finding appropriate statistical methods to obtain reasonably reliable estimates. In our case neither problem was straightforward. Ideally, we would have liked a model to include a variety of factors that might affect the level of terrorism, including advantages and disadvantages of alternative strategies, such as diplomacy, guerrilla warfare, and popular protest, as well as resource constraints, the impact of internecine fighting among PLO groups, and the interaction between governments and terrorists, including responses with concessions as well as coercion. In practice, we could not develop anything close to a fully specified model. Although we would ultimately have liked to develop a more specified model than the 'bare bones' approach we used here, we recognized that some important information, such as reliable data on resource constraints, would never be available to us. Furthermore, although we could, in principle, have used simultaneous equations to model terrorist responses to governments and government responses to terrorists, there was little point in doing so, as available information constrained us to single equation estimation, even if we used a simultaneous system.

Fortunately, there was a solution which allowed us to model the impact of deterrence on terrorism, without having anything close to a fully specified model of the factors affecting terrorism. Since our only reason for trying to avoid 'model misspecification' problems was to obtain reasonable estimates of deterrent effects, we were able to use surrogates to model the impact of a wide variety of variables that we could not measure directly. The technique we used involves the assumption that, at least in short-interval time-series data, the best surrogate for unmeasured causes is the value of the dependent variable at previous time periods.[15] In other words, the assumption was that whatever caused terrorism this month probably caused it the month before, and if there are no very large changes in causes from one month to the next, the level of terrorism in previous months serves as a proxy for a variety of causes that cannot be measured. This argument leads to a model of the following form in Equation 1,

$$Yt = f(Y_{t-1},\dots,Y_{t-m}, X_{t-1},\dots X_{t-m}) \quad (1)$$

In this formulation, Y would refer to the level of terrorism and X to the level of the government's coercive response. Note that this has the additional advantage of incorporating some of the effects of the interaction between terrorism and current government response to terrorism that would appear in a simultaneous equation system.[16] (We should add that we used arrest data as a surrogate for official coercive response, inasmuch as it could incorporate other effects of coercive response such as casualties and incarceration.)

A second major problem that we faced concerned appropriate ways to estimate Equation 1. Time series analysis poses notorious difficulties, such as problems of nonstationarity, serial correlation, difficulties of inference due to inclusion of lagged endogenous variables, and so on.[17] Event-count data pose additional problems, because such data rarely approximate to a normal distribution unless the number of events in each time period is large. Our data were subject to all of these problems. As far as we knew, there is no technique that will eliminate all statistical problems simultaneously, providing unbiased and efficient estimates, although there are ways to eliminate at least some of the most egregious errors that can result from interpreting regression analyses of time-series event-count data. Our strategy was to try a variety of procedures that were reasonable in terms of correcting some of the statistical problems inherent in the analysis. Where different procedures produce discrepant results, we chose to err on the side against our argument. That is, if two procedures appear equally reasonable but do not yield the same interpretation, we focused on the procedure or procedures that demonstrated the highest deterrent effect. We also report alternative procedures that we tried and any discrepancies that exist. Our reasoning here was that if our argument stood up under a wide variety of statistical procedures, including those that yield the least favorable estimates, then the conclusions are unlikely to be simply the result of statistical artefact.

Table 1 presents the results that obtain when the Ordinary Least Squares (OLS) method was used to estimate Equation 1 with a lag of two months. This lag was chosen on purely empirical grounds: it was the highest lag that produced statistically significant results at the .1 level for the lagged endogenous and exogenous variables on any model. The dependent variable, terrorist incidents, was measured in difference form ($DY_t = Y_t - Y_{t-1}$) because there is a statistically significant, although weak, secular time trend in the terrorist data on all measures of terrorism. This trend can be removed by taking first differences of the data. There is, however, no seasonal variation in this data. Thus, first differences allowed us to obtain a dependent variable that appeared to meet conditions for stationarity.[18] Such transformations were not necessary for the arrest data because these exhibit neither seasonal fluctuation nor trend.

TABLE 1

THE IMPACT OF ARRESTS ON PLO OVERSEAS TERRORISM
ORDINARY LEAST SQUARES (OLS) ESTIMATES

	Dependent Variable		
	Total Incidents (DY_t)	PLO Incidents (DY_t)	PLO+ALLY Incidents (DY_t)
Intercept	.45	.31	.34
	(.0383)	(.0285)	(.0296)
DY_{t-1}	–.48	–.61	–.47
	(.0001)	(.0001)	(.0001)
DY_{t-2}	–.17	–.30	–.22
	(.0110)	(.0001)	(.0006)
$Arrests_{t-1}$	–.14	–.24	–.24
	(.0367)	(.0001)	(.0002)
$Arrests_{t-2}$	–.16	–.10	–.07
	(.0157)	(.1100)	(.2780)
R^2	.25	.4	.31
$\hat{q}$	–.01	–.04	–.01
	(.9346)	(.5954)	(.8667)
R^2 (arrests only)	.1	.17	.16
N	213	213	213

Note: This table looks at the impact of arrests on the three different measures of overseas PLO terrorism: all incidents that may have been instigated by the PLO (total incidents), incidents where evidence points to direct PLO involvement (PLO incidents), and incidents where evidence links the incident either to the PLO or to an organization thought to be allied with the PLO in the venture (PLO+ally incidents). In all cases, 'arrests' refers to arrests attributed to the group defined by the dependent variable. The numbers in this table are Ordinary Least Squares parameter estimates and the numbers in parentheses below the estimates refer to statistical significance levels for a two-tailed t test. The $\hat{q}$ toward the end of the table refers to the estimate of the serial correlation of the error term. The R^2 at the end of the table is based on OLS regression with lagged endogenous variables omitted from the equation. This table is based on monthly data of terrorist incidents.

Table 1 indicates that there is a statistically significant deterrent effect of arrests. The previous month's arrests are negatively related to current terrorism on all measures of terrorism. Whether this impact persists past one month is less clear. Total incidents appear responsive to arrests that occurred two months earlier; the remaining measures show negative effects, but these may not be statistically significant.

Due to potential problems of multicollinearity, we also estimated Equation 1 with a lag of one month. These results are reported in Table 2. Several features of this table are noteworthy. The first is that the percentage variance explained by the arrest variables alone in the equation with a two-month lag is identical to that of the equation with a one month lag, for all measures of terrorism. Since this percentage varies from 10 to 17, depending upon which measure of terrorism is used, the impact of arrests, based on the R^2 criterion is quite small. A second is that OLS estimates of the impact of last month's arrests in the equation where only the previous month's arrests are included almost equal the sum of the coefficients in the equation where the past two months are included. (This is particularly true of the PLO and PLO+ALLY equations, where the second lag on arrests is not statistically significant on the usual .05 cutoff.) The OLS estimates in the first two columns of Table 2, in short, indicate that the total deterrent impact (measured as a regression coefficient) of arrests on terrorism is about –.3. If we use this coefficient for all measures of terrorism, its meaning is that it would take an increase of about 300–400 per cent in the arrests of terrorists to cut the number of terrorist incidents by two-thirds.

This estimate of the impact of deterrence may be too large. Although Table 1 does not indicate that serial correlation is a problem with the OLS estimates, potential problems of serial correlation with lagged endogenous variables are serious; in particular, if this occurs, OLS estimates will not even be consistent. For this reason some argue that in cases of doubt, one should assume that serial correlation is a problem.[19] An appropriate estimation procedure under this assumption is Generalized Least Squares (GLS), without lagged endogenous variables.[20] This procedure requires assumptions about the nature of the error term. The most plausible assumption for all measures of terrorism is that the errors constitute an autoregression [AR(2)] process. This assumption is based on the pattern of autocorrelations and partial autocorrelations of the dependent variables and the errors from OLS regression on the arrest variables only. With this procedure, we could only estimate the impact of the previous month's arrests as prior lags are included in the variable transformations.[21]

The Generalized Least Squares (GLS) estimates in Table 2 indicated that the deterrent effects of arrests on terrorism are lower than the OLS estimates would indicate. The R^2 indicate that arrests explain only 7–11 per cent of the

TABLE 2

THE IMPACT OF ARRESTS ON PLO OVERSEAS TERRORISM
LOGGED ORDINARY LEAST SQUARES (OLS) ESTIMATES

	Dependent Variable		
	Total Incidents (DY_t)	PLO Incidents (DY_t)	PLO+ALLY Incidents (DY_t)
Intercept	2.52. (.0001)	2.6 (.0001)	2.52 (.0001)
DY_{t-1}	–.06 (.0001)	–.09 (.0001)	–.07 (.0001)
DY_{t-2}	–.04 (.0001)	–.05 (.0001)	–.04 (.0001)
$Arrests_{t-1}$	–.05 (.0001)	–.06 (.0001)	–.06 (.0001)
$Arrests_{t-2}$	–.00 (.7200)	–.01 (.4395)	–.00 (.8500)
R^2	.36	.47	.40
$\hat{q}$	.09 (.2102)	.05 (.4644)	.00 (.9831)
R^2 (arrests only)	.23	.27	.24
N	213	213	213

Note: The numbers in the tables are logged OLS parameter estimates and the numbers in parentheses below the estimates refer to statistical significance levels for a two-tailed t test. As the data were measured in difference form, a constant was added to each dependent variable to make all values positive. The $\hat{q}$ toward the end of the table refers to the estimate of the serial correlation of the error term. The R^2 at the end of the table is based on OLS regression with lagged endogenous variables omitted from the equation.

variance in terrorism, as opposed to 10–17 per cent in the OLS estimates. The deterrent effect measured by the regression coefficient is also lower: it is about –.2 in the GLS estimates for the different measures of terrorism compared with about –.3 (counting both months in the case of total incidents) in the OLS estimates. This difference translated into further work for government agents: in (say) 1987 it would have taken an increase of about 450–600 per cent in arrests to effect a two-thirds reduction in terrorism. These linear interpolations should not be taken seriously, as there were probably not that many overseas PLO terrorists to arrest. We used them only to indicate the weakness of the deterrent effect we picked up. The regression coefficient and R^2 estimates we obtained for different measures of terrorism and different estimation procedures were substantively in agreement: arrests will deter terrorism, but not enough to reduce terrorist incidents significantly.

We also checked to see whether even the small deterrent effect we picked up is offset by a cycling phenomenon. It is possible that if terrorism responds to arrests, then a high police presence will reduce terrorism for a month or two. However, as terrorism is reduced, police presence is reduced, and as this happens, terrorism then increases. Our data did not indicate that this occurs. We checked for this phenomenon by lagging arrests for 12 months and noting individual and joint effects of the lagged variables. All we could discern was that there was a negative impact of arrests on terrorism that lasted for a month or two and then the impact of arrests disappeared.

We also checked to see whether alternative measures to number of arrests would produce better results. We did not find this to be the case. If the arrest rate is used (the number of arrests divided by the number of incidents) the arrest variables cease to be statistically significant at all.[22] A second possibility we tried was to construct a 'total loss' measure by adding casualties to the number of arrests. This produced results that are almost identical to those of Table 1, although the t values tend to be slightly lower. We also tried a second set of dependent variables measures, constructed by eliminating all cases of violence against Arab targets from each of the three measures of PLO terrorism. We did this because it is possible that PLO attacks on other Arabs were structured differently from attacks against Israeli targets or targets perceived as sympathetic to Israel. As far as response to coercion is concerned, this was not the case. Both parameter estimates and statistical significance levels were very similar to all three models in Table 1. Our conclusion from these alternative measures was that, with an OLS or GLS analysis, coercion exerts a weak deterrent effect on PLO overseas terrorism.

It is, of course, entirely possible that these results were due to the use of linear models in a situation where they are unwarranted. There are several objections to using OLS or GLS on event-count data, including the fact that, at least in the case of violence data, extreme values will dominate estimation. Taking the natural log of the dependent variable may solve this problem if a small constant is added to the dependent variable.[23] The logged OLS model is equivalent to an exponential model. Thus we have:

$$DYt = \exp(DY_{t-1} + DY_{t-2} + X_{t-1} + X_{t-2} + e) \qquad \text{(Equation 2)}$$

or

$$\text{In}DY_t = DY_{t-1} + DY_{t-2} + X_{t-1} + X_{t-2} + e \qquad \text{(Equation 3)}$$

Table 2 presents the estimation results for Equation 3. Although the parameter estimates are different from those of Table 1 (which is required by the change of metric), there is not much difference in the interpretation between the two tables. As in the OLS version, all coefficients of the lagged endogenous and exogenous variables in the logged OLS model have a negative sign. Furthermore, as in Table 1, all are statistically significant, with the exception of the arrest variable measured as the change that occurred two months prior to the current change. [Recall that in Table 1, this variable was not statistically significant on all measures of terrorism.] The R^2 for the logged version of the equation is higher than that of the unlogged version, but this is common with a log transformation. The problem is that the fit is better because extreme values have been changed to values that are less extreme. Thus, for example, a month with 10 terrorist incidents is counted as having 2.3 terrorist incidents. Such a transformation may be eminently reasonable if extreme values are thought to be flukes. But violence data are characterized by periods of no or low activity followed by massive upsurges in activity. One can argue, then, that Table 2 indicates that coercion or the lack thereof can 'explain' as much as a quarter of the variance in terrorist activities. We thought, however, that this estimate was high precisely because extreme values are characteristic of violence data. In fact, it is the extreme value conditions (terrorist events frequently reported in the newspapers) or the dramatic incidents (e.g., hijackings) that generate the public outcry. At any event, even if the results of Table 2 are accepted over those of Table 1, we are still left with three-quarters of the variance in terrorism that arise from sources independent of the coercive reaction of governments. As before, these results cannot be improved by changing the way the independent variable is measured. If arrest rates are used as the independent variables, the coefficients are not statistically significant at the. .1 (or even .2) level for any measure of terrorism. If losses (arrests + casualties) are used instead of arrests only, the results are virtually identical to those of Table 2.

A third estimation possibility is to use models that are not based on normal distribution assumptions. One such alternative that has recently been proposed for political science event-count data is the exponential Poisson regression (EPR) model.[24] This model is similar to that of Eq.(2) in that it expresses terrorism as an exponential function of arrests and control variables. The major difference is that the event counts are assumed to be distributed as a Poisson random variable, rather than a normal random variable.[25] While there is some justification for preferring the Poisson over the normal random variable in the use of event count data, this procedure was also problematic for our purposes. A more serious problem is that event-count data models are restricted to positive integer values for the dependent variable (not unreasonably as event counts are always positive integers). In our case, what this meant is that we could not use the difference transformation to eliminate a time trend (nor, for that matter, could we have used the residuals of a regression on time). The quarterly data do exhibit a statistically significant time trend for some measures of terrorism. Thus we are left with a potential problem of nonstationarity that might be more serious than violations of the assumptions about the correct distribution assumption.[26] This problem can be dealt with in two not very good ways: include time in the regression equation or add an arbitrary constant to differenced data. Although both are subject to statistical problems, in our case it turned out to make very little difference as neither changes the results we derived from a straight EPR analysis.

Table 3 presents the results of this straight exponential Poisson regression: arrests cease to be statistically significant on any measure of terrorism on either the first or the second lag. There is simply no deterrent effect of arrests on terrorism that can be gleaned from this table. To see whether this was due to the form of the regression (exponential Poisson versus logged OLS) we redid the analysis of Table 3 using a logged OLS model on quarterly raw event-count data. The results are presented in Table 4.[27] Again, arrests turned out to be uncorrelated with subsequent terrorism. In fact, despite the different forms of the two exponential models and despite the differences in estimation techniques, model estimates of the impact of prior arrests on terrorism are approximately zero for all measures of terrorism on both models.

It is not entirely clear whether the different results obtained for monthly and quarterly data were due to the level of aggregation or simply to the fact that the monthly data were, for statistical reasons, transformed into first differences, whereas the quarterly data should not be so transformed for the exponential Poisson regression. It is, of course, easy enough to redo the analysis of Tables 1 and 2 on the quarterly, rather than the monthly data, and to transform the differenced data for the analysis of Table 3. The results are relatively unambiguous. If the dependent variables are not logged, but are

TABLE 3

THE IMPACT OF ARRESTS ON PLO OVERSEAS TERRORISM EXPONENTIAL POISSON REGRESSION

	Dependent Variable		
	Total Incidents (Y_t)	PLO Incidents (Y_t)	PLO+ALLY Incidents (Y_t)
Intercept	1.35 (.00)	.56 (.00)	76 (.00)
Y_{t-1}	.06 (.00)	.20 (.00)	.14 (.00)
Y_{t-2}	.01 (.42)	–.03 (.3)	–.01 (.60)
Arrests_{t-1}	.01 (.31)	–.01 (.55)	–.00 (.9)
Arrests_{t-2}	–.01 (.28)	–.02 (.48)	–.01 (.57)
grad*dir	.000000	.000000	.000004
N	71	71	71

Note: This table looks at the impact of arrests on the three different measures of overseas PLO terrorism: all incidents that may have been instigated by the PLO (total incidents), incidents where evidence points to direct PLO involvement (PLO incidents), and incidents where evidence links the incident either to the PLO or to an organization thought to be allied with the PLO in the venture (PLO+ally incidents). In all cases, 'arrests' refers to arrests attributed to the group defined by the dependent variable. The numbers in the tables are numerical approximations to parameter estimates and the numbers in parentheses below the estimates refer to statistical significance levels for a two-tailed t test. The grad*dir number towards the bottom of the table is a measure of how close to a local maximum the approximation came at convergence (it should be exactly 0 at a local maximum). This table is based on quarterly untransformed event data counts.

measured as first differences (as in Table 1), quarterly OLS regression indicates that there are no deterrent effects of arrests on terrorism for any measure of terrorism. This would indicate that it is aggregation rather than differencing that masks any deterrent effects that might be picked up on a finer scale. This conclusion may be supported by our analysis of an exponential Poisson regression where the data are differenced but an arbitrary constant is added so each of the dependent variables takes on integer values. Again we picked up no deterrent effects of arrests on any

TABLE 4

THE IMPACT OF ARRESTS ON PLO OVERSEAS TERRORISM: LOGGED ORDINARY LEAST SQUARES (OLS) ESTIMATES ON QUARTERLY UNTRANSFORMED DATA

		Dependent Variable	
	Total Incidents (Y_t)	PLO Incidents (Y_t)	PLO+ALLY Incidents (Y_t)
Intercept	.91 (.0001)	.06 (.6966)	.29 (.0599)
Y_{t-1}	.12 (. 0001)	.30 (. 0001)	.24 (. 0001)
Y_{t-2}	–.02 (.3928)	–.08 (.2089)	–.07 (.1260)
Arrests$_{t-1}$	.01 (.7521)	–.02 (.6428)	–.01 (.6946)
Arrests$_{t-2}$	–.00 (.8356)	–.01 (.7448)	.00 (.9893)
R^2	.54	.46	.49
$\hat{q}$	–.1 (.4194)	–.14 (.2492)	–.12 (.3428)
N	71	71	71

Note: This table looks at the impact of arrests on the three different measures of overseas PLO terrorism: all incidents that may have been instigated by the PLO (total incidents), incidents where evidence points to direct PLO involvement (PLO incidents), and incidents where evidence links the incident either to the PLO or to an organization thought to be allied with the PLO in the venture (PLO+ally incidents). In all cases, 'arrests' refers to arrests attributed to the group defined by the dependent variable. The numbers in the tables are logged OLS parameter estimates and the numbers in parentheses below the estimates refer to statistical significance levels for a two-tailed t test. The differences between this table and Table 2 are that in this table (a) the data are not transformed by taking first differences and (b) the data are aggregated into quarterly measures. The $\hat{q}$ toward the end of the table refers to the estimate of the serial correlation of the error term.

measure of terrorism, indicating that differencing was not generating the existence of a deterrent effect. However, if the dependent variables are logged (as in Table 2), the results differ for different measures of terrorism – the first time that this problem has appeared. The results are displayed in Table 5, which indicates that total incidents show no response to arrests; and that PLO and PLO+Ally incidents indicate a weak to moderate response to arrests. From all these regressions, it is clear that monthly data are more likely to pick up deterrence effects, and pick up stronger deterrence effects than do quarterly data, regardless of method of estimation. This is true whether parameter estimates, t-tests, or R^2 tests are used as the measure of existence and strength of impact. And, since as noted above, the monthly effects are generally quite weak, we were not picking up very much of a deterrent effect of arrests (or arrests and casualties) on terrorism.

This might, of course, be due to the fact that many terrorists were arrested and then deported or else served little or no time in prisons. Casualty figures are not very revealing on the severity dimension: there are very few deaths of terrorists; the data do not indicate whether casualties are minor or severe. If the arrest variable measures numerous events where people are detained and then quickly released, and the casualty variable is measuring numerous minor wounds, it is not surprising that we managed to uncover few, if any, deterrent effects of arrests or arrests and casualties on terrorism. To investigate this possibility, we eliminated all cases from the arrest data where the terrorists were known to have been deported or to have served fewer than six months in a prison or jail. [This might not be a very strong measure of severity of deterrent effort, but if we make the measure much stronger, we have fewer cases to investigate. As it is, on this measure of severity, the PLO has lost only 7–11 people per year, depending on which measure of PLO activity is chosen.]

Table 6 reports the results that obtained when only 'serious' arrests (those that result in imprisonment of at least six months) were used to assess deterrent effects. We chose to report OLS results rather than logged OLS results because (a) they were easier to interpret, (b) they were based on the actual number of events that occur, and (c) the conclusions on deterrence were not affected by whether or not the dependent variable is logged.[28] We also did not use quarterly data since this weakens the impact of deterrence and since we were unable to obtain a stationary series to analyze in an exponential Poisson regression model.

A comparison of Tables 1 and 6 reveals that there are few differences between the deterrent effect of arrests and that of arrests that eventuate in imprisonment for some significant length of time. Two minor differences are perhaps noteworthy. The first is that in the case of serious arrests, we were slightly more likely to pick up a statistically significant deterrent effect

TABLE 5

THE IMPACT OF ARRESTS ON PLO OVERSEAS TERRORISM: LOGGED ORDINARY LEAST SQUARES (OLS) ESTIMATES ON QUARTERLY DIFFERENCED DATA

	Dependent Variable		
	Total Incidents (DY_t)	PLO Incidents (DY_t)	PLO+ALLY Incidents (DY_t)
Intercept	2.32 (.0001)	1.71 (.0001)	1.0 (.0001)
DYt-1	.09 (.0001)	–.03 (.5410)	.10 (.0003)
DYt-2	.00 (.8969)	–.00 (.8781)	.00 (.9625)
Arrests_{t-1}	–.02 (.2222)	–.05 (.0680)	–.05 (.0271)
Arrests_{t-2}	–.00 (.8795)	–.04 (.2106)	.01 (.6978)
R^2	.35	.27	.28
$\hat{q}$	.05 (.6682)	.36 (.3564)	–.06 (.5961)
N	71	71	71
R^2 (arrests only)	.03	.25	.1

Note: This table looks at the impact of arrests on the three different measures of overseas PLO terrorism: all incidents that may have been instigated by the PLO (total incidents), incidents where evidence points to direct PLO involvement (PLO incidents), and incidents where evidence links the incident either to the PLO or to an organization thought to be allied with the PLO in the venture (PLO+ally incidents). In all cases, 'arrests' refers to arrests attributed to the group defined by the dependent variable. The numbers in the tables are logged OLS parameter estimates and the numbers in parentheses below the estimates refer to statistical significance levels for a two-tailed t test. The difference between this table and Table 2 is that in this table the data are aggregated into quarterly measures. The $\hat{q}$ toward the end of the table refers to the estimate of the serial correlation of the error term. The R^2 (arrests only) at the end of the table refers to the correlation when lagged endogenous variables are omitted from the equation.

TABLE 6

THE IMPACT OF SERIOUS ARRESTS ON PLO OVERSEAS TERRORISM ORDINARY LEAST SQUARES (OLS) ESTIMATES

	Dependent Variable		
	Total Incidents (DY_t)	PLO Incidents (DY_t)	PLO+ALLY Incidents (DY_t)
Intercept	.35 (0.926)	.18 (.1886)	.23 (.1337)
DY_{t-1}	–.50 (.0001)	–.65 (.0001)	–.53 (.0001)
DY_{t-2}	–.19 (.0039)	–.34 (.0001)	–.25 (.0001)
Serious Arrests$_{t-1}$	–.12 (.1588)	–.18 (.0247)	–.19 (.0161)
Serious Arrests$_{t-2}$	–.25 (.0045)	–.15 (.0592)	–.14 (.0777)
R^2	.25	.36	.28
$\hat{q}$	–.00 (.9747)	–.05 (.4539)	–.02 (.8029)
R^2 (arrests only)	.06	.06	.07
N	213	213	213

Note: This table looks at the impact of 'serious' arrests (those that result in at least 6 months' imprisonment) on the three different measures of overseas PLO terrorism: all incidents that may have been instigated by the PLO (total incidents), incidents where evidence points to direct PLO involvement (PLO incidents), and incidents where evidence links the incident either to the PLO or to an organization thought to be allied with the PLO in the venture (PLO+ally incidents). In all cases, 'arrests' refers to arrests attributed to the group defined by the dependent variable. The numbers in the tables are OLS parameter estimates and the numbers in parentheses below the estimates refer to statistical significance levels for a two-tailed t test. The $\hat{q}$ toward the end of the table refers to the estimate of the serial correlation of the error term. The R^2 at the end of the table is based on OLS regression with lagged endogenous variables omitted from the equation.

of arrests that occurred two months earlier. [As was the case with all arrests, there is no impact of arrests that occurred three months earlier.] The second is that the impact of serious arrests, as measured by the percentage of variance that can be accounted for by the arrest variables alone, is even smaller than it was for all arrests. Again, the differences are not large and probably indicate little substantive difference between the deterrent effects of the two variables. But this is interesting, in and of itself. It indicates, in the first place, that the results we got were highly robust with respect to measurement of both independent and dependent variables. And those results consistently indicate that there are weak deterrent effects of coercion on overseas PLO terrorism. These effects, however, are primarily discernible at the monthly, rather than the quarterly, level of aggregation.

This conclusion applies only to the impact of coercion on all PLO overseas terrorist activities in all countries. There was not enough terrorism to analyze the impact of coercion on PLO terrorism in each country separately. However, there were enough instances of terrorism to look at some cross-national statistics to try to answer the following question: even if there is little aggregate effect of coercion on terrorism, will there be a displacement effect so that countries that are 'tough' on terrorists will have fewer instances of terrorism? It may be the case that the PLO engaged in terrorism regardless of coercion, but tried to minimize the impact of coercion by choosing sites that, other things being equal, were least likely to result in arrests and casualties for their members. If this were in fact the case, it would support the general finding about 'substitution effects' argued by Enders and Sandler.[29]

Although the argument in favor of displacement seems plausible, we were unable to discern any deterrent impact from countries that displayed an initially high level of coercive response. Our analysis underlying this answer was based on a division of incidents by country into those that occurred during the first 108 months (9 years, 1968–77) and those that occurred in the second 108 months (9 years). For each country with terrorist incidents in both periods (a total of 27 countries) we calculated the number of incidents,[30] number of arrests, and arrests per incident in the first half of our data and the second half of our data. The cross-national results replicated our earlier dynamic findings with respect to the impact of the rate of arrests: a relatively high coercive response (arrests per incident) in the first period bore no relationship to the number of terrorist incidents in the second period. In fact, the R^2 for this regression was a meagre .004 and the regression coefficient was not even statistically significant at the .75 level! What does predict the number of incidents in a country in the second period is the number of incidents in the first period. In this equation, the regression coefficient is 1.15 (picking up the small positive trend), which is statistically significant at the .000 level. The R^2 for this regression is .58, and remains .58 even if the arrest rate variable is added into the equation.[31]

The problem arises when we compare cross-sectional results with dynamic results on the impact of the frequency of arrests. Recall that in the dynamic analysis, it was consistently frequency, rather than rate, of arrests that exhibited a deterrent effect. In the cross-sectional analysis, the frequency of arrests again emerges as statistically significant, but in the wrong direction! Regression coefficient significance tests, Pearson's r, and Spearman's q yield the same result: countries with high numbers of arrests in the first period had high numbers of terrorist incidents in the second period. The regression coefficient on first period arrests is .792, which is significant at the .01 level on a two-tailed test, with number of incidents during the first period included in the equation. We are unwilling to conclude from this that arrests provoke future terrorism, in part because this contradicts the robust findings of our dynamic analysis and, in part, because there are good methodological reasons for being suspicious of cross-sectional results when the underlying process is dynamic.[32] All we could conclude, then, was that there was no firm evidence that countries that respond to terrorism with higher levels of coercion will (necessarily) be successful in displacing terrorism.

Although there are countries with terrorist incidents in the second 108 months (1978–86) that had none in the first period, the 'high incident' countries tended to remain in the group that was high in the initial period. Thus, while the PLO expanded its range of activities, it still favored countries that were early targets of activity. One plausible explanation for this is that the PLO developed both a network of contacts and knowledge about the terrain of some countries that made it easier for these groups to carry out these activities. They therefore preferred to work on familiar ground whenever feasible. Although the groups aimed to expand the range of targets, it is possible that the need for local assistance, and preliminary investigations in countries that are unfamiliar required greater expenditures of resources than that imposed by even relatively high coercive responses.

Conclusions

We ran numerous regression models to try to overcome serious problems both of measurement of terrorist incidents and of statistical analysis of time-series event-count data. Our feeling was that conclusions that are robust with respect to both measurement and technique are probably valid; those that are not are more questionable. The most robust finding in our analysis was that the frequency of arrests exerts a weak but statistically significant deterrent effect on the frequency of terrorist incidents. This finding applied to all three measures of terrorism used in this analysis and to these measures with Arab targets deleted. It also applied to arrest, serious arrest, and arrest+casualty variables. It emerged on OLS, logged OLS, EGLS, and distributed lag autoregressive models. However, this finding applies only to monthly data. When data are aggregated to the quarterly level, findings are more ambiguous and less likely to yield evidence of deterrent effects. This is perhaps not surprising, as monthly analysis indicates that arrests have an impact for one or two months, and that this impact is fairly weak (our best guess is that it accounts for about 6–15 per cent of the variance in terrorist incidents). A three-month aggregation, then, may be too long a period to pick up consistent results. If this conclusion is correct, it is probably not 'merely' methodological, but rather reflects the empirical reality of the duration of a terrorist response to a coercive incident.[33]

A second negative finding may also be empirically important. In all of our analyses of the impact of coercion on aggregate terrorism (i.e., before we tried to analyze the impact of the reputation of individual countries on terrorism), we picked up an impact only on the frequency, but not on the rate of arrests (or serious arrests or arrests+casualties). This may be a trivial result that stems from incorrect measurement: because we had no way of knowing how many people participated in each terrorist event, we were forced to use arrests/incident as opposed to arrests/number of participants. If, on the other hand, the result does not stem from measurement error, it has substantive implications: it indicates that PLO leaders responded to the number of people who were, to one degree or another, 'lost', rather than to losses per incident. This would seem to indicate that our initial premise was incorrect. That is, despite the few losses to arrests or casualties, PLO leaders are responsive to these numbers. Perhaps this occurs because it took considerable resources to train a terrorist for overseas activities. It may have been far easier to train people for border raids on Israel than to train them to smuggle arms into countries, know the language and terrain of foreign countries well enough to operate successfully in them, and know how to carry out covert operations on unfamiliar territory. In this sense, an overseas terrorist may be more similar to a spy than a soldier, and require concomitant expenditure differences in terms of training and maintenance. This argument is, of course, purely speculative, and was completely beyond our ability to investigate further. We put it forward only because it serves as a plausible explanation for two of our findings. It explains why PLO leaders would be sensitive to the number, rather than the rate of losses. And, it potentially explains why arrests have an impact that lasts for a month or two but not longer, since this would seem to be a reasonable length of time in which to complete the training of a replacement terrorist in the details of the country and operations he or she is to enter.

Our conclusions on the possibility of displacing terrorist incidents by adopting 'tough' policies within individual countries are more tenuous. There are simply not enough instances of overseas terrorism to do much in-depth analysis of subsets of these data. Nevertheless, we found it interesting that, on the face of it, there is no indication that countries that are 'tough' on terrorism are any more successful *in the long term* in eliminating the problem than their more 'spaghetti-spined' counterparts. This again ran counter to our original expectations, but is perhaps understandable if it is the case that the training of a good terrorist is a costly proposition that requires an equally expensive auxiliary network. In this case, PLO leaders probably found it more efficient to back off for a month or two until they could train a replacement than to set up a new network in a different country. This scenario probably offered little comfort to people in countries where the PLO had been relatively active. Moreover, the PLO, over time, expanded the number of countries in which it operated, so it was always possible that regional dispersion would create new targets, new victims, and greater headaches for the governments affected. This turned out to be the case, though (we speculated) the overall PLO effort abroad may have been diffused in its effects because of the higher costs associated with dispersion.

It is also possible that PLO terrorism anywhere will become a moot issue, because the Palestinians will win all or part of their battle for an independent Palestinian state. We did not suggest here that Arafat's renunciation of terrorism in favor of negotiation would spell the demise of overseas terrorism either by disgruntled PLO groups or unaffiliated (pro-) Palestinian organizations. On the contrary: it turned out that, in the short term at least, leaders of organizations opposed to Arafat and his policies increased their use of terrorism to try to undermine his position with various government leaders[34] and to stake out 'hardline' positions of their own. Even more alarming, Muslim fundamentalist groups adamantly opposed to the September 1993 Rabin–Arafat accords mounted their own terrorist campaign both inside and outside Israel.[35] These attacks, apparently designed to undermine or prevent further agreements between the parties and, ultimately, to scuttle the accords, became increasingly numerous as negotiations between Israel and the mainline PLO bore fruit, and (for those within the Israeli–Palestinian territories) remain one of the more vexing security problems facing both parties. Consequently, we are likely to see continued use of terrorism both by some PLO organizations and by other groups such as Hamas, Islamic Jihad, and Hizballah, and for an indefinite length of time.

What, then, should governments do to protect their citizens from the threat of terrorism? Our analysis cannot answer this question, since any answer depends to a large extent on what governments aim to achieve when they adopt policies toward terrorist groups. It is fair to suggest, however, that most governments should adopt anti-terrorism policies that are designed at least as much to reassure their own constituents as they are to controlling, mitigating, or eliminating terrorism. Such policies would entail treating most incidents as ordinary crime, but responding to a few dramatic incidents with an equally dramatic coercive response or concessions, depending on what government leaders perceive popular sentiment to want. (The latter strategy, at least, if Enders and Sandler[36] are right about dramatic coercion, appears to provide substantial political payoffs for the governments involved, and often leads terrorist groups to seek substitute actions and venues for their operations.) Auxiliary policies might entail engaging in preventive measures, such as airline security checks, where such measures are economically feasible and given realistic concerns about terrorism. Our analysis indicates that such policies may be eminently reasonable. PLO overseas terrorism resulted in injuries or, less commonly, deaths to about 67–122 people per year. While any injury or death is deplorable, these casualty figures must be taken in context with other socio-economic problems that produce their own victims, impoverish the lives of citizens, and consume scarce public resources.[37] The parameter estimates of Table 1 indicate that it would take about a 600 per cent increase in the frequency of arrests to effect about a two-thirds decrease in the frequency of terrorism. This linear extrapolation should not be taken seriously, as there probably are – or were – not that many PLO or Palestinian-related terrorists engaged in overseas actions to arrest. But the figures do indicate the cost of eliminating terrorism through arrests or arrests and casualties: either governments must pay considerably more money to increase the number of police and other security personnel, decrease the fight against other crimes, shift resources from social and medical programs to police and military ones, or find ways other than arresting and physically interdicting terrorists to fight terrorism. While some have suggested other ways – including attacking Middle Eastern countries that support groups currently engaging in terrorist acts or assassinating leaders of such groups – no one has been able to demonstrate that such alternatives would be either effective in eliminating or vastly reducing terrorism, or 'efficient' in terms of the price paid in the loss of Arab (and possibly non-Arab) good will.

Preventive measures are feasible only in some cases. Some evidence indicates that airport security measures have been successful; there is also evidence suggesting that terrorists respond to such measures by substituting 'cheaper' measures.[38] The economic havoc wrought in 1978 by the poisoning of some Israeli oranges sent to European markets vividly illustrates the difficulties of finding workable preventive measures to curb terrorism. Governments can install metal detectors at airports, surround the president and other high officials with security officers, and defend other prominent people and places. But it is too costly to install detection devices in every place of business or surround every citizen with a network of security officers. In the final analysis, if preventive measures only cause terrorists to substitute other forms of terrorism rather than to abstain from terrorism, prevention is unlikely to solve the problem.

Our analysis indicates that official coercive response is also unlikely to offer much of a solution. PLO overseas terrorism was somewhat responsive to government coercion. In general however, transnational terrorism, whether inspired by the Palestinian cause or not, is unlikely to be eliminated or significantly reduced by coercion, however, both because the causes of most terrorism lie largely outside the purview of government coercion, and because governments lack the resources necessary to track down all terrorists and their replacements. This means that, to the extent that terrorist acts confer benefits such as publicity, encouraging further terrorism, one-upping other terrorist groups, winning support of constituents, and so on, we are likely to be stuck with the problem indefinitely. Where political solutions are feasible as a way of reducing terrorism, as appears to be the case at least with the mainline PLO and Israel, and (in 1994–96) the ceasefire and subsequent negotiations with the IRA in Northern Ireland, they must obviously be sought. But there remain nexuses of conflict (such as those involving the Kurds) seemingly impermeable to resolution attempts, causes generating demands that are nearly impossible to meet (e.g., the Armenians' against the Turkish state), and groups clinging to extreme or apocalytic solutions (such as the Palestinian hardliners and religious fundamentalists). With respect to terrorism generated by such situations and groups, governments have no choice but to remain on guard, and to employ such countermeasures, both passive and coercive, as will deflect, mitigate, or diminish the threat to their citizens. Our analysis supports the view that, unfortunately, such measures can have only partial, and then probably only temporary, effect.

NOTES

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10. IDF, *Spokesman*, 'International Terror July 1968 – July 1986' and ibid. (Oct. 1981), 'The Sabotage Organizations: Thirteen Years of Terrorism Abroad, 1968–1981'. This is available on request from the Israel Defense Forces *Spokesman*. We would like to thank Dr Alon BenMeir for his assistance in locating this source.
11. E.F. Mickolus, *Transnational Terrorism: A Chronology of Events, 1968–1979* (Westport, CT: Greenwood 1980); idem, T. Sandler and J. Murdock, *International Terrorism in the 1980s: A Chronology of Events*, 2 vols. (Ames, IA: Iowa State UP 1989).
12. IDF (note 10). In some cases, the precise date of the arrest is not noted. In these cases we assume that the arrest occurred during the month of the offense. This assumption is justified by criminal research which indicates that, in the vast majority of cases, arrests are made at the scene of the crime or shortly thereafter. This, of course, might not be true of the highly dramatic terrorist incidents, such as the kidnapping of athletes at the 1972 Olympic Games, but such incidents account for a very small percentage of total terrorism. Furthermore, they may take so long, as in the trial in 1989 of Fawaz Yunis, who was abducted in 1987 and brought to the US to face charges involving a 1985 hijacking, that the deterrent effect may be highly attenuated. After all, terrorists do not lead very secure lives, and the threat of a jail sentence several years away may not have much impact on people who willingly face the threat of a more imminent demise.
13. A. Merari and S. Elad, *The International Dimension of Palestinian Terrorism* (Boulder, CO: Westview Press 1986).
14. IDF (note 10). A few incidents were excluded when there was little indication that they were terrorist in nature, e.g., the mugging of an Israeli diplomat in Washington DC.
15. C.W.J. Granger and P. Newbold, *Forecasting Economic Time Series*, 2nd ed. (Orlando, FL: Academic Press 1986).
16. We assumed here that a simultaneous system would have the police responding to current terrorism (in the same month), while terrorists would take more time to respond to the impact of past arrests. For further justification for this in a different context see C.W. Kohfeld and J. Sprague 'Demography, Police Behaviour, and Deterrence', prepared for delivery at the annual mtg of the American Soc. of Criminology, LeGrand Hotel, Montreal, Quebec, 11–14 Nov. 1987.
17. J. Johnston, *Econometric Methods*, 2nd ed. (NY: McGraw-Hill 1972) pp.243–432 and G.G. Judge *et al.*, *The Theory and Practice of Econometrics*, 2nd ed. (NY: Wiley 1985) pp.224–416.

18. C. Chatfield, *The Analysis of Time Series: An Introduction,* 3rd ed. (London: Chapman & Hall 1984) pp.21, 35–7; Judge, ibid. pp.232–3.
19. Johnston (note 17) p.258.
20. Judge (note 17) pp.293–6.
21. Since the EGLS procedure offered us no way of checking the correct lag length of the arrest variables, we also checked the model by using Almon's method for distributed lag estimation under the assumption of an AR(2) process. Under this assumption, the first and second lags are negatively related to all measures of terrorism, and are statistically significant at the .002 level or better. Parameter estimates range from -.10 to -.12 for each of the two month's previous arrests for all measures of terrorism. Higher order lags do not emerge as statistically significant.
22. This may be because we should measure arrest rates by the number arrested divided by the number of active terrorists. However, we had no way of knowing how many people participated in each of the terrorist events in a given month.
23. The addition of a constant is necessary because the log function is defined only for positive numbers. In our case, as the data are in the form of first differences, we added a constant that made the lowest value of the dependent variable equal to .1 (We chose –.1 instead of the more usual .01 to avoid reintroducing the problem of extreme values at the low end of the function.)
24. G. King, 'Statistical Models for Political Science Event Counts: Bias in Conventional Procedures and Evidence for the Exponential Poisson Regression Model', *Amer. Pol. Sci. Rev.* 32/3 (Sept. 1988) pp.838–63.
25. We used the SST program to estimate model parameters. This program assumes that the dependent variable has the probability function:

    $$\mathrm{Prob}(Y_i=y_i)=(e^{-k_i}k_i^{y_i})/y_i!$$

    where log $k_i = x_i'b$ and x_i are the independent variables. The program uses numerical methods to maximize the joint likelihood function $y_1,\ldots,y_n$. For further information about this technique see J. Hausman, B.H. Hall, and Z. Griliches 'Econometric Models for Count Data with an Application to the Patents – R&D Relationship', *Econometrica* 52/4 (Dec. 1984) pp.909–38.
26. Chatfield (note 18) pp.35–41 and Judge (note 17) pp.275–98.
27. In the logged OLS model, serial correlation does not appear to be a major problem. In the standard OLS version, serial correlation seems more significant. Nevertheless, in the standard version, no coefficient on either of the arrest variables for any measure of PLO terrorism is statistically significant at as conservative a level as 20.
28. In the case of serious arrests, there is not even much difference in the R^2 of the logged and the unlogged OLS results. If the dependent variable is not logged (see Table 6) arrests account for 6–7 per cent of the variance in terrorist incidents. If the dependent variable is logged (not shown), arrests account for 6 per cent of the variance in terrorism for all measures of terrorism.
29. See note 2.
30. We used the most inclusive measure of PLO incidents for this analysis to get as many instances as possible. We felt this was reasonable, given the fact that prior analyses showed little sensitivity to differences in dependent variable measurement.
31. To see whether these results were due only to a few countries with high levels of PLO activity, such as West Germany, France, and Italy, we ranked the variables and redid our calculations using Spearman's q as our measure of association. On this measure, the relationship between coercion in the first period and incidents in the second was positive but very small (q=.13). The relationship between incidents in the first period and incidents in the second was also positive, but quite substantial (q=.83). Both OLS and rank order correlation methods, then, yield the same results, which are that the PLO units have not moved from one country to the next in response to government coercion of their activities, but rather have tended to maintain a high presence in countries that were initially selected as targets.
32. G. Tsebelis and J. Sprague, 'Coercion and Revolution: Variations on a Predator–Prey Model', *Mathematical Computer Modelling* 12/4–5 (1989) pp.547–59; see also C.

McCaughrin, 'Statics and Dynamics of Dissent', *Comparative Political Studies* 15/4 (Winter 1983) pp.405–23.

33. We were unable to discern whether the duration of the impact should be one or two months, because different models yielded different results on this question. Note, though, that a two-month duration on a single equation model may simply reflect a one month lag in a simultaneous terrorist/police interaction model. That is, if the police respond to last month's terrorism, as well as current terrorism, and terrorists respond only to last month's arrests, we will pick up a two-month lag in a single equation model.
34. See e.g. S. Mishal, *The PLO under Arafat: Between Gun and Olive Branch* (New Haven, CT: Yale UP 1986); D. Schiller, 'A Battlegroup Divided: The Palestinian Fedayeen', in D.C. Rapoport (ed.) *Inside Terrorist Organizations* (NY and London: Columbia UP and Frank Cass 1988) pp.90–108. The latter is also available in *Jnl of Strategic Studies* 10/4 (Dec. 1987) pp.90–108.
35. For their aims and activities, see Z. Abu-Amr, *Islamic Fundamentalism in the West Bank and Gaza* (Bloomington, IN: Indiana UP 1994).
36. Enders and Sandler (note 2).
37. For some comparisons indicating the relative insignificance of terrorism, see T. Sandler and J.L. Scott, 'Terrorist Success in Hostage-taking Incidents', *JCR* 31/1 (March 1987) pp.35–53; and D.K. Shipler, *Arab and Jew: Wounded Spirits in a Promised Land* (NY: Penguin 1987).
38. Sandler and Scott, ibid. and Enders and Sandler (note 2).